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" *To the solid ground*
Of Nature trusts the mind which builds for aye."—WORDSWORTH

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HYDRAULIC EXPERIMENTS

Roorkee Hydraulic Experiments. By Major Allan Cunningham, R.E. Three vols. (Roorkee: Thomasen College Press, 1880-81.)

UNDER the direction of the Indian Government there have been constructed a number of canals, which, while reaching in transverse dimensions a size not much inferior to the Suez or North Sea Canal, have a far greater length and ramify into smaller channels of enormous total extent. Besides these, reservoir and river works have been carried out of the greatest magnitude. Hence the Indian Government has a most direct interest in the advancement of the knowledge of hydraulics. Not only must hydraulic formulæ be used in the design of hydraulic works, but also in regulating the distribution of a valuable commodity—irrigation water—on which large revenues depend. Yet down to a recent period the Indian Government has been content to avail itself of researches carried out in Europe, and chiefly in France, and has made no use of its splendid opportunities for scientific hydraulic experiments. When at last hydraulic experiments on a large scale were sanctioned, involving a large expenditure it was very fortunate that the direction of them was intrusted to so very competent an officer as Major Cunningham. "Beaucoup de personnes croient que tout homme intelligent et instruit peut faire, sans grand travail de bonnes experiences; c'est une erreur qui a fait perdre beaucoup de temps et d'argent." So says M. Boileau, who is himself one of the most careful of hydraulic experimenters. Major Cunningham certainly does not think lightly of his work. He has enormous industry; he repeats his observations again and again; he studies every detail of his methods; he notes the opinions of all his predecessors in work of a similar kind, and discusses his results with great lucidity. If his experiments have furnished no strikingly novel laws, the fault is not his.

"The general result of this work may perhaps be considered in some ways disappointing, in that there are no brilliant results, no simple laws of fluid motion discovered, not even a new formula for mean velocity proposed" (p. 4).

It is certainly true that when Major Cunningham passes from discussing the details of practical methods, where he is always instructive, to purely scientific questions, to generalising laws from the results obtained on verifying accepted rules, he has a rather exceptional number of purely negative conclusions to state. It is almost amusing to find caution carried to the extent involved in printing as a general result of a considerable discussion that the value of the coefficient in the formula for the discharge of a stream "depends *probably* on the nature of the banks and bed, as well as on the hydraulic mean depth and slope." But nevertheless we believe the practical objects of the experiments have been obtained, and the outlay usefully incurred. Less of thoroughness at all events would have rendered the experiments useless, and although considering the scale of the experiments they seem at present rather less fruitful of definite results than might have been expected, yet it may be hoped that Major Cunningham has not made the most that can be made of his results. In time the new suggestion will come which will reduce to order the discordant observations. In the establishment of any new general conclusions or formulæ in the hydraulics of streams, this store of data will certainly be of the greatest value.

Of the magnitude of the work undertaken by Major Cunningham, it is difficult to give an adequate idea. It lasted over four years. The results include 565 sets of vertical velocity curve observations, each set including velocities taken three times at each foot of depth; 545 sets of rod float observations, each including six measurements of velocity; 581 sets of mean velocity observations, each including three measurements of velocity at ten to twenty points; 440 measurements of surface slope; besides many others. In addition to all this, the tabulation and computation of the results involved enormous labour. The printing of the results at Roorkee, whilst it must have involved greater trouble and responsibility than similar work in this country, seems to have been most efficiently and accurately done.

From the practical point of view Major Cunningham's book may be regarded as an exhaustive treatise on Float Gauging. All the more important observations were

made by floats, and he has used these simple instruments in all their known forms, as surface floats, sub-surface floats, twin floats, and rod-floats. Every detail of the construction and use of these floats has been studied, their form, the length of run, the mode of marking the sections and float paths, and the precautions in taking the time. The sources of error are weighed, and in some degree the limits assigned beyond which the methods become unreliable. There will always be cases where the methods of float-gauging must be used, and no one who has work of this kind to do can afford to neglect Major Cunningham's directions. A few observations were, in fact, made with current meters. But the instruments used were of a type which must now be regarded as antiquated, and as to these Major Cunningham suggests no improvement which has not already been tried by the German engineers, who have, in fact, converted the current meter into a new instrument of precision.

It is not at all to be regretted that Major Cunningham adopted floats in his experiments. Even from the scientific point of view, if floats are at best a rough means of determining velocities, yet they are not liable as more complicated instruments are, if used without sufficient care or knowledge, to large and concealed errors. Hence float observations may always be used advantageously to check observations made in other ways. The progress of hydraulics suggests questions, for the solution of which float methods are inadequate, and the results obtained by Harlacher and Wagner seem to show that floats will be superseded by instrumental means of greater complication, but of far greater delicacy. But in truth in hydraulics no one method is free from objections and researches carried out by all methods, when sufficient care is exercised, will prove useful.

We may now pass to consider briefly the bearing of these experiments on some points of theory. Major Cunningham devotes Chapter VI. to a discussion of the unsteadiness of the motion of the water in ordinary streams. At each point the velocity varies in direction, and magnitude from instant to instant. The float-velocities taken on 50-feet runs, which are themselves mean velocities for a certain time and distance, vary from 10 to 30 per cent., so that to obtain the true mean velocity over any given float-path, something like fifty float observations are necessary. Recent current-meter observations show this variation of velocity still more clearly. The essential unsteadiness of the motion of water in streams was pointed out with the greatest clearness by M. St. Venant (1872), and the still more important fact that the motion is periodically unsteady, that is, that the variations occur periodically about a constant mean value, so that the average velocity for a sufficient but not very great length of time is sensibly constant. It is only this last fact which has rendered it possible, to apply the equations for steady motion to the actual motion of streams, and it is a pity that Major Cunningham has not adopted St. Venant's convenient term, mean local velocity, for the sensibly constant average velocity at each point of a stream. It is not the "interlacing of the stream lines" (p. 107), but the destruction of stream-line motion by eddying motions of quite another character, to which the unsteadiness seems to be due.

In Chapter VII. the observations of the surface-slope at different periods of the experiments are discussed, and it is here that we think may be discovered the one matter in which the conditions of the experiments were unsatisfactory, and in which they are markedly inferior to Bazin's small-scale experiments. Taking the Solani embankment and Solani aqueduct sites, at which the largest amount of work was done, we find that the experiments were made at about the centre of a ten-mile reach, terminated at the upper end by a regulator controlling the water-supply, and at the lower end by a fall where, by artificial means, the water-level was kept up to any desired height. The bed of the canal between these limits had originally the uniform slope of about a foot per mile. This original level is maintained at five points by masonry works, but between these the bed is irregularly scoured out to an extent which must have made very sensible variations of velocity within distances of a mile. At the tail of the reach is a weir standing five feet above the level of the bed, the crest of which was further raised by temporary obstructions of a height sometimes reaching five feet more. Hence the whole height of obstruction was often greater than the whole depth of water at the site of the gaugings. Under these circumstances the slope of the water surface varied, being generally quite different in the part of the reach above the site of the experiments from that in the part below, where the influence of the tail weir was felt. Further, the difference of slope in the parts of the reach above and below the site of the experiments differed widely in different conditions of the water supply. The local surface slope, that is the slope of the water surface in the neighbourhood of the gauging site, varied irregularly with the variation of the slopes above and below, being apparently, as might be expected, most affected by the obstruction at the tail of the reach. Now as the velocity at a given site does not exclusively depend on the surface slope at the site, but to a certain extent on the slope above and below, the conditions of the site were initially to some extent unfavourable, and that in a degree which, although it may be small, is difficult to appreciate. The local surface slope itself can only be measured on a considerable length of stream (1000 to 4000 feet). But in that length the surface slope appeared to vary, the slope in 2000 feet being as much as 25 per cent. different from that in 4000 feet, and the slope at one bank being 50 per cent. different from the slope at the other. It is obvious, therefore, that the local surface slope is a quantity which, under the conditions of these experiments, was not ascertainable with any great accuracy. But the whole comparison of the experimental results with formulæ of discharge involves the accurate knowledge of this quantity. All inferences from these experiments as to the reliability of formulæ must be weakened in proportion as the slope measurement is doubtful.

It is not in Major Cunningham's experiments alone that this difficulty in determining the surface slope has been found. It is to the uncertainty of this quantity mainly, to this *fons et origo malorum*, that the discordances of large-scale experiments are due. The roughest small-scale experiments, those, for instance, discussed by Eytelwein and Prony, have furnished coefficients more useful in practice and more generally applied than any large-scale experiments hitherto carried out. The advan-

tage of regular canals over natural rivers for hydraulic experiments almost disappears when the canal bed is scoured out to an irregularity similar to that of a natural stream, and the canals are at a disadvantage when artificial control at the tail of the reach modifies the conditions of flow to an extent sensibly felt at the site of the experiments. It is, of course, in the lower states of the water in the canal in the Roorkee aqueduct reach, that the effect of the tail control is most sensible, but then experiments made in these conditions are an essential part of the data necessary for generalisation.

Major Cunningham spent a good deal of time in verifying a supposed theory that the surface of a stream should be convex. The theory is probably a capital instance of the frequent mistake of importing the principles of theoretical hydrodynamics into practical hydraulics. In a stream flowing from a reservoir, in such a way that the tangential forces on the surface of the elementary streams are absent or negligible, the energy per pound of fluid is uniformly distributed. It follows that in parts where the velocity is greater, the pressure is less. A stream may be regarded as a bundle of horizontal filaments coming from a common reservoir. If in such a stream the central filaments have a greater velocity than those nearer the sides, their pressure will be less. Consequently, for equilibrium there must be a greater depth of stream towards the centre, and the transverse water-line will be convex upwards. Such is the theory which Major Cunningham has taken a great deal of trouble to test, and to which he attaches weight in spite of his observations. From preliminary calculations he shows that the known differences of velocity would give a difference of level, between the centre and sides of the Ganges Canal, of 3 inches. After the most careful measurements, it was found that the difference of level varied from +0·018 foot to −0·095 foot, the average difference being almost exactly zero. Obviously the theory is outrageously wide of the truth, and the reason is not far to seek. The differences of velocity to which the supposed differences of pressure are due, are created by exactly those tangential actions of the filaments which the theory neglects. There is no reason for assuming equal distribution of energy along a filament, part of the energy of which is being destroyed by lateral frictional actions between the filaments. As to the observations in Chapter V., with a guage giving still water-level, it is not clear that the small difference of level observed was not due to the position of the mouth of the tube which communicated with the canal.

The discussion of the vertical velocity parabolas in Chapter XI. is extremely interesting, and the method adopted for finding the most probable curve by the method of least squares, is laborious and conscientious. The method of weighting the observations seems, it is true, rather artificial, especially as the observations at great depth best define the form of the parabola. The general conclusion arrived at is, that while all the observations can be fairly well expressed by parabolic curves, no formula can be found expressing the dependence of the variation of velocity on the slope, and dimensions of the channel. It would be interesting to see if a parabola with axis on the water-line would not agree better with the results, the observations above the line of maximum velocity being of course discarded. So far as there is any theory of the mutual action of the filaments, it leads to the result that the parabolic axis should be at the surface; and that is not inconsistent with one possible explanation of the reduction of velocity near the surface.

In ordinary streams, the velocity is greater towards the surface and centre, and less towards the bottom and sides. But the greatest velocity is not found at the surface, but at a variable depth below it, amounting very often to one-fourth of the whole depth. The Mississippi observers attributed this to the friction of the water against the air. In accordance with this they found the depression of the line of maximum velocity to depend quite directly on the direction of the wind, and they logically introduced into their formulæ of flow, the free surface, as forming part of the frictional wetted border. Major Cunningham retains the Mississippi observers' explanation, while his experiments disagree with theirs on all the points which directly support the explanation. He finds, for instance, that the depression of the line of maximum velocity is entirely independent of the direction and force of the wind. Now excepting one suggestion to be referred to presently, no kind of retarding action between the air and water has been stated which is not of the nature of a frictional resistance. The Mississippi observers and some others who adopt the explanation of the depression of the line of maximum velocity we are now criticising, state explicitly that they consider the resistance between the air and water to be of the same nature as the resistance between the water and its solid bed. If so, since the line of maximum velocity is ordinarily depressed to one-fourth the depth at the centre, and generally still more towards the sides, the friction between the water and air must be something like one fourth as great as the friction between the water and solid bed. But is it conceivable that the friction between the level water surface and mobile air should have anything like one-fourth the value of the resistance of the water impinging on all the immovable roughnesses of the stream bed? Further, any resistance of this kind must depend on the relative velocity of the water and air. But the air is most commonly in motion, and on the average must as often and as long blow down stream as up stream. Blowing down stream, it should accelerate the stream to the same extent as blowing up stream it retards it. But it is known from Boileau's experiments and others that the depression is still persistent with a wind blowing down stream at a velocity greater than that of the water. To this Major Cunningham's only answer is that "the time required for the penetration of change of velocity of the surface current caused by wind to any considerable depth appears to be very great. It has been estimated that it would take one week for half change of surface velocity to penetrate three feet." The evidence for this is not given, but if it is so, is it not because the friction between air and water is extremely small, and it is only in those cases where the persistence of the wind action for a long time allows an accumulation of effect, that that effect is sensible.

A wind blowing on the surface of a lake is long in producing a current merely because the friction is small, but it does produce a current in time, because the action is cumulative. On a river it produces no sensible effect at all, as Major Cunningham's experiments show. But if

the friction between air and water is as great as he supposes, it ought to produce a sensible effect, and since winds blow as often and as long down-stream as up-stream, the water-surface should as often be accelerated as retarded, and the vertical velocity parabola should as often have its axis above the water-surface as below it. Boileau does indeed suggest that the absorption of air by the water and the evaporation of the water cause a loss of energy near the surface, but here again the cause seems as inadequate as air-friction. The experiment of Francis, quoted on p. 107, is admitted by Major Cunningham, to prove that "there is a continual transfer of water from the bed towards the surface, even in water in apparently tranquil motion," and his own float-observations (p. 269) show that "near the edge of a stream there is a persistent flow of the water at and near the surface from the edge towards the centre." Now the flow from the bottom and sides towards the top and centre brings water, stilled by impinging on roughnesses of the bed, to replace the quick moving surface-water. It is not true that the water so rising must acquire the velocity of the layers through which it passes, for it may rise in eddying masses, which are but little affected by the friction on their surface, or the motion of the water may be in horizontal spiral paths, which allow the bottom water to reach the surface without passing through the quicker moving central parts of the stream. At all events the transfer of the bottom water to the surface is a known phenomenon, and it is adequate as an explanation of the diminution of surface velocity.

In Chapter XVI. is given a somewhat elaborate theory of the motion of a rod-float, which leads to the result that the rod-velocity is slightly less than the true mean velocity of the water past the immersed portion of the rod. Quite apart from the question of the general unsteadiness of the motion of the water, it may be pointed out that the relative velocity $v-u$ of the streams impinging on the rod must for the most part fall below the limit for which the pressure due to impact or friction can be assumed to vary as the square of the velocity. Hence the calculation that the rod-length should be 0·94 of the depth of the water to give a true mean velocity, seems an extremely doubtful one.

In criticising thus two or three points of theory, it must be pointed out that these matters do in fact lie somewhat outside the main objects of the experiments, and an error on these points detracts nothing from the practical value of Major Cunningham's work. W. C. U.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

"Weather Forecasts"

WILL you permit me to call attention to the apparently complete failure of the Forecasts of Weather given in the daily papers with respect to the storm of Tuesday, October 24? The matter seems to me to be one of much practical moment. Here is an extract from the "Weather" article in the *Times*, which I presume agrees with that given in each of the daily papers:—

Forecasts of Weather for Tuesday, October 24 (*issued at* 8.30 *p.m. on the previous day*).

0. SCOTLAND, N.—South-westerly breezes, fresh or moderate; showery.
1. SCOTLAND, E.—South-westerly breezes, moderate; some showers, with bright intervals.
2. ENGLAND, N.E.—Same as No. 1.
3. ENGLAND, E.—Same as No. 5.
4. MIDLAND COUNTIES.—Same as No. 1.
5. ENGLAND, S. (London and Channel).—Westerly and south-westerly breezes, light to fresh; fine and cold at first, some local showers later.
6. SCOTLAND, W.—Same as No. 0.
7. ENGLAND, N.W. (and N. Wales).—Same as No. 0.
8. ENGLAND, S.W. (and S. Wales).—South-westerly winds, fresh to strong; showery.
9. IRELAND, N.—Wind returning to south-west, and freshening; weather showery.
10. IRELAND, S.—Same as No. 9.

Warnings.—None issued.

By order,
ROBERT H. SCOTT, Secretary.

Notice particularly the concluding words: "Warnings; none issued;" and then remember what took place. It is curious to compare in this respect the *Times* of October 24 with that of October 25. In the latter issue we read as follows:—

"Yesterday morning a violent gale of wind, accompanied by a heavy downpour of rain, visited London. The previous night was beautiful, but at three o'clock yesterday morning the sky became overcast, and from half-past four o'clock up to ten o'clock there was an incessant downpour of rain. At half-past nine o'clock the upper part of 19, Windmill Street, King Street, New Cut, was stripped off, and the occupiers of the upper floors had a narrow escape. At ten o'clock a sign-board was carried away from the frontage of a house in Jewry Street, Aldersgate Street. Although the street was crowded, no one was reported hurt. At Five Fields, Dulwich, the grass was strewn with broken arms from the trees, and a large elm at Norwood was blown down. A portion of a large shed situated near the Surrey Gardens Estate was unroofed. The trees in the various metropolitan parks have suffered severely from the gale. The River Thames at ten o'clock resembled a small sea, and much damage was done to the shipping below London Bridge."

And much more to the same purpose.

I feel desirous of knowing, both on general and scientific grounds, and also for obvious practical reasons, whether any explanation can be given of this absolute breakdown of weather science. It would seem to be possible that a storm can visit our coasts, and do immense destruction both by sea and land, and yet not give the faintest notice to our weather prophets of the impending danger; and it really almost makes one smile to perceive that on the day of the storm no warnings were issued, and that on the day after "the South Cone was hoisted this morning in Nos. 2, 3, 5, 7, and 8."

If no mistake has been made in the observations, and a mistake seems scarcely possible, we seem to be driven to the conclusion that a storm of the first magnitude can come upon us unawares; and if this be so, the conclusion is discouraging and very strange as regards science, and it is very serious as affecting the value of forecasts of the weather to fishermen and others.

I write this letter with the hope that some light may be thrown upon the subject to which it refers. H. CARLISLE

Rose Castle, Carlisle, October 26

The Comet

I BEG that you will allow me space for a few lines of comment upon the letters and drawings of the comet in your last issue, my own included. While thanking the engraver for the generally accurate reproduction of my sketch, it is clear that wood-engraving scarcely admits of a perfect rendering of stumped shading. A few words of correction will serve all the purpose of preserving for possible future use the evidence which I wished to put on record. The chief defect is in the *isolation* given to the "wisp," described by another correspondent as a "horn." It seemed rather to be an inclined elongation of the brightest part. The inclination too is exaggerated: its prolongation should have passed *within* the star on the northern [1] border, but

[1] The tail lies nearly along a parallel of declination.